

ment" of the memoir by one of us (M. D. S.) as well as by other competent critics.

As regards 3, namely, the criticism of the memoir which we give on pages 245-7 of the new edition of our book, *Alcohol and the Human Body*, he says of it, "this is purely a rhetorical production," etc., and that it is not "serious treatment" of the memoir.

If your readers will kindly refer to our book, pages 245-7, they will find that we there give the same reasoned objections to the memoir we have both of us stated in this correspondence and elsewhere.

III.

One point has arisen clearly out of this discussion, and that is that the second half of Professor Pearson's material—namely, the manuscript account of the children in the special schools of Manchester, prepared for him and Miss Elderton by Miss Mary Dandy—should be published. It has been constantly referred to in the columns of our JOURNAL, and, as scientific material, therefore ought not, in our opinion, to have been kept private. We invite Miss Dandy to make this contribution to science at once, and particularly as we wish to see whether her facts have suffered at the hands of Miss Elderton and Professor Pearson the same treatment, which we are about to show in a detailed paper, these authors have applied to the report of the Edinburgh Charity Organization Society.—We are, etc.,

MARY D. STURGE.
VICTOR HORSLEY.

December 27th, 1910.

SIR,—As a matter of handling of statistics, Professor Karl Pearson seems to have completely answered his critics, yet no one with much practical medical knowledge can doubt that he is wrong in his conclusions.

Where the fallacy lies does not appear to have been pointed out, yet it seems fairly evident. The materials used are derived from a decidedly "submerged" population. The causes of the submergence must be very various and numerous, and in each individual instance probably multiple.

We must assume that the forces they exert are fairly uniform in amount throughout the mass, their tendency to produce idiocy in the next generation will therefore be fairly uniform also.

Take x as representing the combined force of all these causes, the two groups have as such forces in Group 1—simply x , left unexamined and unanalysed. In Group 2 we have x analysed into forces due to alcohol and others (unexamined).

The weak point in Professor Pearson's case appears to be that he says

Forces and causes in Group 1 = x .

Forces and causes in Group 2 = $x + \text{alcohol}$.

The real facts are:

Forces and causes in Group 1 = x .

Forces and causes in Group 2 = $x = y + \text{alcohol}$.

The forces y act in both groups. In Group 2 + alcohol. In Group 1 + other forces not represented, or less strongly represented, in y . These are largely no doubt various mental and physical disabilities.

Professor Pearson does not prove that alcohol has no effect in producing idiocy, but only that it has no more effect than some other causes that are of equal effect with alcohol in producing "submergence."—I am, etc.,

Reigate, Dec. 24th.

T. A. CHAPMAN.

ANAESTHETICS ACT.

SIR,—I wish fully to endorse the opinion which Dr. W. J. McCardie has expressed in his letter to the JOURNAL of December 10th, wherein he deplores the action of the Anaesthetics Committee of the General Medical Council in recommending that a dental surgeon shall be allowed to administer one or more of drugs, specified in schedule, for operations other than dental conducted by a legally qualified medical practitioner.

The committee apparently make no attempt to show the necessity for granting any such licence, which in spirit would be quite antagonistic to the main principles of the bill—namely, the protection of the public by providing that anaesthetics shall be administered only by such members of the community as have undergone a training

calculated to render them efficient to administer these drugs with the greatest possible degree of safety.

The mere administration of nitrous oxide (probably the first drug to be included in the schedule) for dental purposes is an art in which proficiency can be fairly easily acquired without any previous medical or surgical training. But the administration of this drug for prolonged surgical procedures presents quite a different aspect, and is at times, even in experienced hands, beset with such difficulties as to call for special clinical knowledge on the part of the anaesthetist, or even for the application of one or more of the more dangerous anaesthetics if the surgical procedure in question is to be conducted with any degree of satisfaction to the operator or of safety to the patient.

The recommendation of the committee would almost certainly entrust nitrous oxide anaesthesia to a person not possessed of that clinical experience which alone could enable him to (1) discriminate between suitable and unsuitable subjects, (2) appreciate danger signals early enough to avoid disaster, (3) deal with danger when it arrived.

The adoption of the recommendation would surely be a retrograde step in a forward movement; it would stultify, in a great measure, the prevailing teaching—that a sound clinical training is essential to safety in anaesthetization.

It would tend to produce a condition of mental anorexia in the medical student, who would feel ill disposed to apply himself seriously to the study of a subject which the law could treat with such indifference. It requires no great stretch of imagination to picture the suggested concession as forming the thin edge of a wedge the base of which might be represented by a school for teaching the art of anaesthetizing to lay members of the community who had had no preliminary training in medical subjects.

As the progress of surgery is so directly dependent upon advances made in the study of anaesthesia it would be interesting to hear comments from surgeons upon the suggested alteration in Clause 1 of the Anaesthetics Bill.—I am, etc.,

Clifton, Bristol, Dec. 12th.

ARTHUR L. FLEMING.

THE CAUSE OF APPENDICITIS.

SIR,—In the JOURNAL of December 3rd, there are three letters on the cause of appendicitis, and one by Dr. Rainsbury asks for information relative to the frequency of appendicitis in India.

For six years I was in charge of fifteen mission hospitals in the native State of Travancore, in the extreme south of India, and I have notes that, of 1,123 major operations performed during this time, only 10 were for appendicitis. The year after I left, my successor records 553 major operations by himself and Indian assistants, with no cases of appendicitis. This conclusively shows a rarity of that condition in that part of India at least.

It may be of interest further to notice that out of the 10 cases recorded, 7 were merely appendiceal abscesses, and of the remaining 3, 1 was complicated with salpingitis, which furnished the only death in the series. The second was a simple recurrent appendicitis with no adhesions or evidence of trouble, and the third was in a neurotic youth who was not much improved by the operation.

Referring to Dr. Maclean's letter, I may say that tuberculous disease is common, tonsillitis rare, and acute rheumatism unknown. Then Dr. Gurney mentions influenza as a possible cause. This is a disease we never saw.

I may say that I have two or three pamphlets by Indian practitioners in favour of the idea put forth by Dr. Rainsbury, claiming that the freedom of the Indian from appendicitis is due to the natural position assumed in the act of defaecation.—I am, etc.,

Southport, Dec. 19th.

WM. C. BENTALL.

THE BRADSHAW LECTURE ON CANCER.

SIR,—The parasitic theory of cancer in the sense that cancer is due to a specific extrinsic living organism is quite untenable; but the existing evidence nevertheless points to the probability that cancer may sometimes be propagated by a *contagium vivum*—that is, by living cancer cells, direct descendants of the living body cells of